DISCUSSION:

POPPER'S VARIANT OF THE EPR EXPERIMENT DOES NOT TEST THE COPENHAGEN INTERPRETATION*

A. SUDBERY

Department of Mathematics
University of York

In the "Preface 1982" to his *Quantum Theory and the Schism in Physics*, Karl Popper (1982a; see also 1982d) has proposed an experiment that is designed to test the Copenhagen interpretation of quantum mechanics. By "the Copenhagen interpretation" Popper appears to mean the theory of quantum mechanics as generally presented in current textbooks, e.g., Dirac 1958 (if I am wrong about this, Popper's assertions about the Copenhagen interpretation are of less interest). Here I shall follow normal usage and refer to this generally accepted theory simply as "quantum mechanics." Using the set-up of Einstein, Podolsky, and Rosen (1935), Popper claims that quantum mechanics predicts striking effects which would be a clear demonstration of instantaneous action at a distance. In this note I hope to show that Popper was wrong in thinking that these effects are a consequence of the theory. This illustrates a general theorem of Ghirardi et al. (1980) that shows that such effects can never be predicted by quantum mechanics. I shall also draw attention to some inconsistencies in Popper's attitudes to quantum mechanics that seem to be evident in this preface and other passages in *Quantum Theory and the Schism in Physics*.

Popper considers two separated particles A and B which have "interacted in the past" (more realistically, which were produced together) and whose properties are correlated in such a way that a measurement of the y-coordinate of A gives information about the y-coordinate of B, and similarly a measurement of the y-component of momentum of A gives information about the y-component of momentum of B. Such will be the case if the wave function of the particles is

$$\psi(\mathbf{r}_A, \mathbf{r}_B) = \phi_1(x_A)\phi_2(x_B)\delta(y_A - y_B) \tag{1}$$

where ϕ_1 and ϕ_2 are separated localized wave packets. This is an adaptation of the EPR wave function (Einstein et al. 1935); it represents separated particles that have equal values of y and opposite values of p_y.

*Received July 1984.

Philosophy of Science, 52 (1985) pp. 470–476.
Copyright © 1985 by the Philosophy of Science Association.

More carefully stated, it follows from the Copenhagen interpretation of the wave function that if y_A is measured and found to have a certain value, a measurement of y_B will certainly give the same value; and a similar statement holds for p_y with "same" replaced by "opposite."

Popper proposes passing each particle through a slit of finite width Δy in the y-direction and then measuring its momentum p_y in this direction by recording its arrival at one of an array of detectors arranged around the slit (fig. 1). If this is repeated a large number of times, the values of p_y will show a spread (or "scatter") that is related to the width of the slit by the uncertainty relation

$$\Delta y \cdot \Delta p_y \geq {}^1/_2\hbar. \tag{2}$$

This holds separately for A and for B.

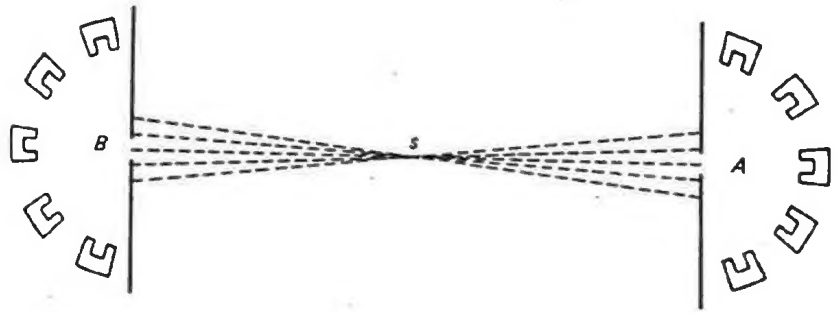

Fig. 1 (Popper 1982a, p. 28)

Now suppose A's slit is narrowed while B's slit remains at a fairly wide setting. Then because of the properties of the wave function (1), the increased knowledge of y_A immediately yields increased knowledge of y_B, so Δy_B is reduced even though the slit has not been touched. This, Popper argues, causes an increase in the spread Δp_{yB} by virtue of the uncertainty relation (2). Thus there is an observable difference in the behavior of B's side of the apparatus—previously unaffected counters start to register—without any physical agency having acted in this region. Let us call this effect E.

The essential ingredients of this deduction are: (i) the inverse relation between Δy and Δp_y, the uncertainties in position and momentum; and (ii) the correlation between the positions of two particles that have interacted in the past. Neither of these is universally true, whatever interpretation of quantum mechanics is in question; each of them holds only in certain special circumstances. According to the Copenhagen interpretation, they do not hold simultaneously; hence Popper's deduction of the effect E is not valid within this interpretation.

To explain my assertion that (i) and (ii) are not universal features of quantum mechanics, I will consider them separately. As to (i): there would always be an inverse relation between Δy and Δp_y if the uncertainty relation (2) were an equation rather than an inequality. As an inequality, however, it is consistent with such an inverse relation but does not entail it. In the case of a particle going through a slit, it is possible to make the width Δy of the slit smaller without increasing the spread Δp_y in momentum, if Δp_y is already large when the slit is wide open. This is precisely the situation if the particles are described by the wave function (1) when they are incident on the slits. The uncertainties Δy_A, Δy_B, Δp_{yA} and Δp_{yB} calculated from this wave function are all infinite; thus, narrowing slit A has no effect on the range of momenta of the particles at either A or B, since these ranges cannot be increased any further. The particles approach A and B at all angles, and all the counters register throughout the experiment.

If changing the width of the slit at A is to affect the range of momenta of the particles after they have passed through the slit, they must approach the slit with a small range of momenta. This indeed appears to be what Popper has in mind, since he proposes to "consider pairs of particles that move in opposite directions along the positive and negative x-axis." If this is to mean that the momenta of the particles are precisely directed along the x-axis, so that the y-component of each momentum is zero, then it is true that only central counters will register when the slit at A is wide; and narrowing the slit will cause higher and lower counters to come into play. But in this case the wave function of the particles is not (1) but

$$\psi'(\mathbf{r}_A, \mathbf{r}_B) = \phi_1(x_A)\phi_2(x_B) \tag{3}$$

which does not have the properties of the EPR wave function.

This brings us to consideration of (ii), the correlation between the positions and momenta of the particles. This is not a general property of particles that have interacted, but a special property of particles that have interacted in the particular way that results in the wave function (1). Moreover the correlations will be destroyed if the particles are disturbed by the apparatus so as to change the wave function. In particular, suppose the particles, after being produced with wave function (1), are collimated so as to reduce the spreads Δp_y, as considered in the last paragraph. This can be done by collimating just one beam, which will demonstrate the correlation between the momenta of the particles, but it will destroy the correlation between their positions. The wave function will now be (3), and a measurement of y_A will give no information about y_B. Thus there is no justification for the statement, essential to Popper's argument, that narrowing the slit at A will cause new counters to register at B.

To summarize: If the particles approach the slits in the EPR wave function, so that observation of one particle gives information about the other, then the spread of the counters that register particles does not depend on the width of the slit. Conversely, if the experiment is arranged so that the spread of counters does depend on the width of the slit, the observation of one particle gives no information about the other.

In this experiment the predictions of quantum mechanics can be understood by supposing, as Popper would like to, that each particle has definite values for both y and p_y, and that the uncertainty relation (2) is a statement about how these values must be scattered statistically in any collection of particles that can be produced experimentally. No action at a distance need be postulated: the correlation between the values of y (or p_y) determined on the two sides of the experiment can simply be understood by supposing that when the particles were produced they had correlated values for both y and p_y. (This is no surprise: it was after all from the conjunction of the proposition that there is no action at a distance with the predictions of quantum mechanics, that is, the existence of the correlations, that Einstein, Podolsky, and Rosen argued that simultaneous values of y and p_y must exist. The argument would have lost its force if the premises had been inconsistent.) In general, these three ideas (the predictions of quantum mechanics, the idea that every measured quantity has a definite value before it is measured, and no action at a distance) *are* inconsistent; but to bring out the inconsistency one needs three incompatible observables. That is why the discussion (Clauser and Shimony 1978; Wigner 1970) and the experiments (Clauser and Shimony 1978; Aspect, Grangier, and Roger 1982; Aspect, Dalibard, and Roger 1982) have concentrated on the phenomena of spin and polarization.

Popper argues that because "spin is really something very queer" (Popper 1982a p. 24), it is possible that it really is affected by action at a distance over small distances, but not over large distances. This, he thinks, distinguishes an experimental set-up relating to spin from the original EPR set-up relating to position and momentum, for which large distances are essential. It is not clear whether polarization is infected by the queerness of spin; but in any case the argument carries little weight. The only reason why large distances are needed in the EPR experiment is to enable the experimental arrangement to be changed between the separation of the particles and their arrival at the detectors. The polarization version of the experiment has been done (Aspect, Dalibard, and Roger 1982) over distances as large as this; there is therefore no reason to distinguish between this experiment and the original EPR experiment.

The queerness or otherwise of spin does not affect the logical position. From the hypothesis that a particle always has definite simultaneous values for all the properties that can be determined by experiment, together

with the hypothesis that there is no action at a distance, one can deduce by elementary reasoning (Wigner 1970; d'Espagnat 1979) certain empirical consequences ("Bell's inequalities") that conflict with the experimental facts. One of the hypotheses must therefore be false. Popper, wishing to retain the first, is bound to reject the second hypothesis and accept the reality of action at a distance. Quantum mechanics denies the first hypothesis; it therefore remains possible that the Copenhagen interpretation is consistent with the denial of action at a distance. This is indeed the case, at least in the sense that if quantum mechanics is true no *observable* effect can propagate instantaneously (Ghirardi et al. 1980).

It is hard to see why Popper should be so determined to maintain the first hypothesis, since it is at best an awkward companion to views that he has defended vigorously in other questions. I will list these, with a minimum of analysis, in the following paragraphs. It will be easiest to discuss the hypothesis in question in the form in which Popper himself usually discusses it, namely, that a particle always has simultaneous values of position and momentum. For ease of reference I call it the "hidden variables hypothesis."

1. *Testability*. A particular statement of the form "this particle now has position x and momentum p" will, if subjected to experimental test, usually be found to be false. A general statement of the form "all particles prepared by experimental procedure E have position x and momentum p" will, if thoroughly tested, certainly be found to be false; for the tests include measurements of position and measurements of momentum, and it follows from the uncertainty relation that one of these is sure to give falsifying instances. Thus the attribution of simultaneous values of x and p to a particle can be held to be true only by being protected from testing (as suggested, for example, by Reichenbach [1944]), that is, only by ceasing to be a scientific statement.

Both in *Quantum Theory and the Schism in Physics* and in *The Logic of Scientific Discovery* Popper is concerned to discover what Bohr and Heisenberg can have meant by the statement that a particle cannot have simultaneous values of x and p. It is strange that the man who taught us how to demarcate scientific statements from others should not have interpreted the pronouncements of Bohr and Heisenberg in this light. On p.61 of *Quantum Theory and the Schism in Physics* he refers to the possibility of testing statements about single quantities, without seeing that such tests would never be passed by statements about two quantities simultaneously.

2. *Realism*. The hidden variables hypothesis is sometimes called "realism"; more commonly, theories incorporating simultaneous values of

position and momentum are called "realistic" theories. I have avoided this terminology, because it seems to me quite inappropriate to use the vocabulary of ontology in a discussion of whether things have this or that particular set of properties. There seems to me to be no more reason to think that a real particle must have a definite position and momentum than there is to think that it must have a definite color and smell. (Rather less, the concept of velocity being puzzling to the intuition—as Zeno found when considering the arrow.) Popper insists (Popper 1982a, p. 103) that by physical understanding he does *not* mean the building of models nor "working with pictures or metaphors which may give us a feeling of familiarity or ease"; but it is hard to see what other motive lies behind his attachment to position and momentum.

3. *Propensities*. To attribute simultaneous position and momentum to the particles of quantum mechanics is to assimilate them to the particles of Newtonian mechanics, which is, in Popper's terms, a "prima facie deterministic theory" (Popper 1982b). To a physicist this gives rise to a prima facie contradiction between the hidden variables hypothesis and the general principle of indeterminism, to which Popper declares his allegiance. In fact the only contradiction is between the common physicists' (and others') usage of the word "indeterminism" and Popper's (the sense intended by Popper might be better expressed by "unpredictability"). However, there does seem to be a substantive contradiction between prima facie determinism and Popper's propensity interpretation of probabilities (Popper 1982c). In a prima facie deterministic theory, the necessity for a probabilistic statement about an individual event arises from (I do not say it can be reduced to) lack of knowledge of the conditions before that event (and, perhaps, logical restrictions on calculating power). In this case, talk of propensities sounds odd: there is no occasion to talk of the propensity of a particle to *take up* certain states (Popper 1982a, p. 126) if it is already in a definite (but unknown) state and there are no quantum jumps (Popper 1982a, p. 135).

In quantum mechanics there is a distinction between two kinds of probability: they can be described as the probability that something is the case and the probability that something will happen. The first kind is relevant when the system is in a "mixed state," requiring the use of a density matrix; the second kind is the only kind to be considered when the system is in a "pure state," described by a state vector. (The language used shows that both kinds are, or can be, regarded as objective probabilities.) A prima facie deterministic theory (like a hidden variables theory) reduces the second kind of probability to the first, but in a nondeterministic theory like quantum mechanics such a reduction is impossible; a statement of the probability of an event will in general have an irreducible residue. It

is this residue that is particularly well described in terms of propensities: a particle does not in general have definite values of position or momentum but it does have propensities to take up particular values of either.

REFERENCES

Aspect, A.; Dalibard, J.; and Roger, G. (1982), "Experimental Test of Bell's Inequality Using Time-varying Analyzers", *Physical Review Letters 49*: 1804–07.
Aspect, A.; Grangier, P.; and Roger, G. (1982), "Experimental Realization of Einstein-Podolsky-Rosen-Bohm Gedankenexperiment: A New Violation of Bell's Inequalities", *Physical Review Letters 49*: 91–94.
Clauser, J. F., and Shimony, A. (1978), "Bell's Theorem—Experimental Test and Implications", *Reports on Progress in Physics 41*: 1881–1927.
d'Espagnat, B. (1979), "The Quantum Theory and Reality", *Scientific American 241* (No.5): 158–81.
Dirac, P. A. M. (1958), *The Principles of Quantum Mechanics*. Oxford: Clarendon Press.
Einstein, A.; Podolsky, B.; and Rosen, N. (1935), "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?" *Physical Review 47*: 777–80.
Ghirardi, G. C.; Rimini, A.; and Weber, T. (1980), "A General Argument Against Superluminal Transmission through the Quantum Mechanical Measurement Process", *Lettere al Nuovo Cimento 27*: 293–98.
Popper, K. R. (1982a), *Quantum Theory and the Schism in Physics*. London: Hutchinson.
———. (1982b), *The Open Universe*. London: Hutchinson.
———. (1982c), *Realism and the Aim of Science*. London: Hutchinson.
———. (1982d), "Proposal for a simplified new variant of the experiment of Einstein, Podolsky and Rosen" in *Physik, Philosophie und Politik*, Meyer-Abich (ed.). Munich: Carl Hanser.
Reichenbach, H. (1944), *Philsophic Foundations of Quantum Mechanics*. Berkeley and Los Angeles: University of California Press.
Wigner, E. P. (1970), "On Hidden Variables and Quantum Mechanical Probabilities", *American Journal of Physics 38*: 1005–9.

BOOK REVIEWS

RUTH GARRETT MILLIKAN. ***Language, Thought, and Other Biological Categories: New Foundations for Realism.*** Cambridge: The MIT Press/A Bradford Book (1984), xi + 355 pp. $29.95.

Call it exhilarating, call it exasperating, this book is both. Undaunted by the risks of sweeping speculation, Professor Millikan takes aim at a breathtaking range of philosophical problems and fires away with vigor and aplomb. Opponents fall like clay pigeons, while solutions appear out of thin air.

The tendentious title is explained by a sustained appeal to a neo-Aristotelian notion of "proper function." Millikan claims that the meanings of words and the contents of thoughts depend on their "natural history." Every type of word or thought has something that it is "supposed to" do, even if it does not do so in fact. Tokens of each type are members of a "reproductively established family," and because they perform their function they proliferate and the family survives. How they have performed their function historically is given by a "Normal explanation." One device after another is said to "perform its proper function in accordance with a Normal explanation," but despite the many allusions to Normal explanations, I could not find a single articulated example of one. The reader can only wonder how Normal explanations are supposed to work and what analogy there is, if any, between how they work in language and how they work in biology. An obvious disanalogy is that whereas tokens of each biological type are produced by tokens of that type (their immediate ancestors), tokens of a linguistic type are not—they are not produced by linguistic tokens at all. Millikan recognizes the difference between these two kinds of reproduction ("first-order" and "higher-order" reproductively established families) but downplays the difference when pushing the analogy with natural selection.

The notions mentioned above are but a sample of the rich battery of evolution-inspired notions that Millikan develops. Her definitions strike me as the most original and cogent aspect of the book, and I wish she had worked them into a systematic account of functional and evolutionary explanation. That would have been of great value in itself, and would have given more content to the analogy with language and thought. Instead, the reader is treated to such *obiter dicta* as: "If man is a natural creature and a product of evolution, it is reasonable to suppose that man's capacities as a knower [and all other capacities?] are also a product of evolution (p. 7), and "economy dictates that traits serving no purpose are highly likely to disappear" (p. 27), as if there is no difference between a trait's surviving because adaptive and its disappearing because maladaptive.

Millikan suggests that every linguistic device has its own proper function without which it would not survive, at least not in accordance with a Normal explanation. The indicative mood, for example, has the function of producing true beliefs, and the imperative mood the function of producing compliance. There is no mention of the interrogative, the subjunctive, or the nearly extinct (because maladaptive?) optative mood. She also proposes proper functions for expressions of various types, generally without considering what linguists or philosophers have said or how they might object. For example, her suggestion that the proper function of the indicative mood is to produce true beliefs is open to the obvious objection that clauses of conditionals, "that"-clauses, and all sorts of other embedded clauses can be in the indicative mood. This mood seems to function Normally in these contexts, but without performing the function of producing (true) belief. Besides, even when belief is produced, it is a perlocutionary effect, not the essentially communicative (illocutionary) effect of understanding. Millikan also neglects the distinction between sentences and utterances, and as a result she sometimes attributes to the one properties that belong to the other.

Despite its sweeping scope and controversial contentions, the book maintains a certain cheery innocence. For the most part it is unencumbered by more than passing mention of prominent views on its main subjects: philosophy of biology (function, functional explanation, evolution), philosophy of psychology (concepts, content, functional role), philos-